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AMERICAN METEOROLOGICAL JOURNAL.

A Monthly Review of Meteorology, Medical Climatology and Geography.

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AMERICAN METEOROLOGICAL JOURNAL.

AN ILLUSTRATED MONTHLY

DEVOTED TO SCIENTIFIC METEOROLOGY AND ALLIED BRANCHES OF STUDY.

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The editor solicits communications on all subjects having any bearing on Meteorology or American Geography. Correspondence on events of current interest is invited, and accounts of local newspapers concerning them will be welcome. Notes on local climate will be published and the columns of the JOURNAL are freely open for worthy communications on all matters within its scope. The editor does not hold himself responsible for any communications which may be signed or attributed to other authorship than his own.
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THE AMERICAN

METEOROLOGICAL JOURNAL.

VOL. II. ANN ARBOR, JANUARY, 1886. NO. 9.

CURRENT NOTES.

WANTED, A NEW CLINICAL THERMOMETER.—C. E. Warren, M. D., writes to the *Scientific American* complaining of the fragility of the glass thermometer and asking for one less destructible. Such a thermometer should be accurate for the range between 90° and 110° F., and must have the degrees divided into fifths or tenths. The scale must be easily readable, and the instrument must be of convenient size and shape. If cylindrical it should not be more than six inches long; if oval, not larger than a robin's egg. The mechanism must be so enclosed as to be impermeable to moisture, but easily accessible for cleaning. The problem is certainly not a difficult one, and for the invention of such an instrument there would be abundant reward.

OZONE.—We publish in this number by an eminent chemist a brief article on the tests for ozone. The dissatisfaction often felt by meteorologists with ozone observations is not due to a depreciation of the value of exact knowledge on this subject, but to a conviction that the test-reaction with Schœnbein's paper is due to other constituents of the atmosphere together with ozone. The information which Professor Prescott gives will therefore be welcome. As Dr. Nicholson's article has attracted very general attention, we take the opportunity to say that his address is Boyne City, Michigan, to enable correspondents to communicate with him directly.

Rise of Water at Chicago.—On Friday, December 4, a storm caused a rise of water on the lake-front of eight feet above the ordinary line. The same storm was very destructive to shipping, and it washed away a mile of the sea-wall at Sand Beach, Michigan. The gale was at first from the north, but changed at about the time of the highest rise to the northwest.

Among the papers presented at the thirteenth annual session of the American Public Health Association, held at Washington, Dec. 8–11, 1885, was one by Dr. Henry B. Baker on the "Relations of Rainfall and Water Supply to Cholera." In his paper, Dr. Baker shows that since 1870 the mortality from cholera in Calcutta has been reduced two-thirds from what it was previous to that year. Since the year 1870 good water supply has been introduced into that city, and the reduction of mortality from cholera seems to bear direct relation to that fact. Previous to 1870 the mortality from that disease fluctuated with the amount of rainfall year after year, the less the rainfall the greater the prevalence of cholera.

Can We Change the Climate of New England?—Several proposals have appeared in the newspapers lately to change the climate of New England by changing the Arctic ocean current which comes down parallel to the coast. One proposal is to deflect the current away fron the coast by damming the Straits of Belle Isle. Another is to pass the current through the Straits, but to warm it up in its passage by expending work on it (turning water-wheels, paddles, etc., in it). The possibility of these processes is a legitimate subject of discussion, but before spending time on them, it would be well to inquire whether, if practicable, they would have the desired effect. The following consideration indicates that they would not. Our changes of weather and, therefore to some extent, our climate, come from the west and northwest. The climate of New England would depend largely, therefore, on the regions to the west and northwest, and as their climate is severe that of New England must partake of its rigor. The proper place to apply a furnace for heating all New England would be to the westward, and the furnace must either warm up the great plains

to the west or the region of the great lakes over which the most of the storms reaching New England have previously passed.

LIGHTNING AND WIRE FENCES.—It is almost impossible to read the newspapers without being impressed with the fact that fatal accidents by lightning are more common, and where particulars are given, these occurrences seem to owe something of their increase, at least where live stock is concerned, to the increased use of wire fencing. It has been observed that several of the reports of deaths to live stock by lightning have referred to the victims as standing by the wire fencing. The frequency of such cases suggests the vicinity of the fence as being as dangerous in case of a storm as proximity to trees or a haystack. The erection of a low shed in the pasture, away from trees or fence, would afford a shelter that animals would soon learn to avail themselves of, and which would probably be free from the disadvantages attaching to the fence.—*Live Stock Journal.*

NATURAL LIGHT IN OFFICES.—In the *School of Mines Quarterly* for October, 1885, is an interesting discussion by James L. Greenleaf on "Measurement of Light Obstruction." Now that the elevated railways are a permanent part of New York city, the problem of the obstruction of light has become a practical one and has got into the courts. Mr. Greenleaf has studied it practically, and in this paper describes his methods and gives his results. There is space here only for some reference to the latter, though the former are ingenious and worthy of imitation. The amount of light cut off by the road alone was, in two cases studied with especial care, 15 per cent. to 29 per cent. for the basement and 11 per cent. to 22 per cent. for the first floor. For the road with train on it, the ratios were 39 to 44 and 28 to 33 respectively. The eye is not good at estimating the intensity of diffused illumination, and the amounts mentioned above might be cut off without its being especially noticeable to an inattentive occupant of the rooms. It is, nevertheless, objectionable, and is a proper subject for a suit for damages.

NEW ENGLAND METEOROLOGICAL SOCIETY.—The annual meeting of the Society was held at Boston, on October 20th. The Council

presented a report of the work of the year, Professor Davis read a paper upon the thunder-storms of the summer of 1885, and Mr. Harold Whiting a paper upon the self-recording aneroid barometer. The report of the Council was a full presentation of the work attempted during the first year of the Society's existence. For the information of all interested, the following summary of this report is given:

During the past year the Council has engaged in the following branches of work: (1) the securing of a corps of reliable observers of meteorological phenomena, with special attention to precipitation and temperature; (2) the publication of the monthly bulletin; (3) the dissemination of the daily indications of the United States Signal Service, and the local display of weather flags; (4) the special investigation of thunder-storms. The work of securing reliable observations was so far advanced in November, 1884, as to warrant the issue of the first bulletin for that month, and its regular publication thereafter. The first bulletin contained reports from forty-five observers; that for September, 1885, from one hundred and twenty-three observers. Efforts have been constantly made to secure increased accuracy and greater uniformity in the observations, in order that they may furnish a reliable basis for future investigations. The subject of accurate instruments received early attention; after many experiments it was decided to manufacture a special class of rain gauges, rather than to adopt any now in the market, and to adopt for self-registering thermometers those of certain reliable firms. The experience of the year has fully justified this policy. All who were desirous of making observations have been encouraged to do so, irrespective of location, but efforts have have also been made to secure observers in special localities. This would be an easier task if the Society were able to furnish instruments, for the cost of instruments has sometimes prevented those interested from undertaking the work. The bulletin has been prepared on a uniform plan, with a few minor changes suggested in the progress of the work; in addition to the usual summary of the observations, each number has contained a map, prepared by the Secretary, showing the distribution of precipitation, and temperature ranges.

The Society has co-operated with the United States Signal Ser-

vice in the dissemination of the daily indications, and the local display of weather flags. A member of the signal corps, Mr. O. N. Oswell, has been assigned to duty under the Society's control; without his assistance this portion of the work would not have been undertaken. The aid thus given has been of great advantage in other departments of work, especially in the clerical labor attending the preparation of the bulletin, and the special investigation of thunder-storms. The Signal Service observer at New Haven has also successfully labored to secure the display of weather flags in Connecticut in harmony with the Society's plans. As a result local weather flags are daily displayed in more than one hundred cities and towns of New England.

The special investigation of thunder-storms was made under the supervision of the Secretary. The members of the National Academy, who constitute the trustees of the Bache fund, appropriated $200 for this work. More than four hundred observers co-operated, the largest number of reports for any single storm having been two hundred and three. The preliminary study of the reports thus far made indicates that some interesting results have been obtained, which will be reported upon subsequently.

The original membership of the society was 9; the number at the close of the year 95. The expenses of the Society have been kept within its income, but this has been done through the generous co-operation of friends, who have from time to time contributed liberally to its resources.

In looking forward to the work of another year the Council suggests that special efforts be made to add to the membership of the Society, as well as to the list of observers. It must be remembered that the financial prosperity of the Society depends on the number of members. It is desired to include in the membership all who are interested in meteorological studies in New England, whether they make observations or not. A member need not be an observer, nor is it required that an observer shall be a member. The Council expresses the hope that the second year of the Society may be as successful as the one just ended, and sincerely thanks all who have contributed to its prosperity.

The New England Meteorological Society is devoted to the

study of atmospheric phenomena in New England. It desires to include in its membership all those who are interested in Meteorology, whether or not they make observations of any kind. Meetings for the discussion of meteorological topics are held on the third Tuesdays of October, January and April, at places designated for each meeting. The annual membership fee is $3.00. The bulletin is published monthly, and contains a summary of the meteorological conditions of the preceding month, with other items of interest. It is furnished without charge to members and co-operating observers. Observations are welcomed from any one residing in New England or in that portion of New York east of the Hudson River. Correspondence relating to the Society and to membership, and to the display of weather flags, should be addressed to the Secretary, W. M. Davis, Cambridge, Mass. Correspondence relating to the current observations should be addressed to the Director, Winslow Upton, Providence, R. I. The Society will be glad to receive monthly reports of temperature and rainfall made with trustworthy instruments, especially from persons residing at points not yet represented on the map. Instructions and blanks will be furnished on application to the Director.

The Council of the Society is composed of the following gentlemen: Wm. H. Niles, Mass. Institute of Technology, Boston, Mass., President; Wm. M. Davis, Harvard College, Cambridge, Mass., Secretary; Desmond Fitz Gerald, M. Am. Soc. C. E., Brookline, Mass., Treasurer; E. B. Weston, M. Am. Soc. C. E., Providence, R., I.; Winslow Upton, Brown University, Providence, R. I., Director.

REVIEW OF EUROPEAN WEATHER FOR OCTOBER.—*Barometric pressure*—The barometric pressure is this month, almost everywhere, considerably below the normal. The month opens with an extensive minimum in the N.W., (barometer at Sumburgh Head on the 1st 28.80. This depression recedes on the 4th but another quickly takes its place and is situated on the 7th near Copenhagen, after causing severe storms in the British Isles and northern Germany this minimum has reached on the 8th the northern parts of Russia and there disappears: another follows and on the

10th is centered near Hurst Castle (Southampton) where the barometer has fallen to 28.80 on the 11th. This depression spreads over Germany and is divided into three minima one over west another over NE. Germany and the third over Italy and the Adriatic. High pressure now advances in the west on the 13th, there are only two depressions, one over the North Sea and another over the Baltic near Riga. A high pressure is also central in west Russia on the 15th, the minima just mentioned have disappeared and the maxima in the west and east joined together so that high pressure is now general over North, West, East and Central Europe. There is only a low pressure visible over the Mediterranean, causing high winds, much precipitation and high temperature over the Adriatic.

On the 10th a minimum appears in the S.W. but recedes on the 18th; a small depression over the Baltic disappears in the N.E. High pressure is now general in the N.W. France, Germany and south Russia with calms but cloudy sky. On the 19th again a depression in the S.W. appears and on the 20th has reached Italy. Another from the N. has traveled on this date to the Baltic near Memel, causing snowfall over Sweden and Norway; this disturbance disappears in the N.E., but another advancing from the Atlantic has pressed down the maximum over Britain so that low pressure is now general over the greatest part of Europe. On the 22nd, however, the minima have lost in intensity so that the pressure is very equally distributed. Frost is by this time general in Scandinavia, Russia and east Germany; on the 23rd a maximum has appeared over north Scandinavia with severe frost, while a depression advances from the S.W. and on the 24th has reached south England: it depresses, however, on the 26th on the approach of a very intensive minimum in the N.W. (barometer at Sumburgh Head 28.73) this depression spreads over the greatest part of Europe, its center being situated on the 28th near Skagen on the north coast of Denmark. On the following day three minima are visible one over northwest Germany, another over south Sweden and a third over Finland. A maximum advances from the west and on the 30th has reached Norway, while another appears in the south, but a disturbance from the Atlantic quickly follows and is situated on the 31st near the entrance of the canal

between England and France, so that on this date two regions of low pressure, one in the west and another in the east, are separated by a small zone of high pressure reaching from north Scandinavia to southern Italy. The temperature in Germany has by this time reached the freezing point.

Temperature.—The temperature of this month was also very much below the mean; in Holland it was lower than was ever observed before.

Germany,—below the mean 2-6, 8-15, 19-24, 26, 28-31; above the mean 1, 7, 16-18, 25, 27, lowest on the 22nd and 31st at Munich 29½°, highest on the 16th, at Breslau 72°.

Ireland, Valentia,—below the mean, 1-2, 5, 11-16, 18-25, 27-29, 31; above the mean, 3-4, 6-10, 17, 25, 26, 30, lowest on the 22nd, 39°, highest on the 6th, 55°.

Russia, Petersburg,—below the mean, 1-2, 18-25, 30-31, above the mean, 3-17, 26-29, lowest on the 24th 14°, highest on the 11th, 57°.

Sweden, Stockholm,—below the mean, 5-11, 15-31, above the mean 1-4, 12-14, lowest on the 22nd 23°, highest on the 3d and 12th 54°.

Lapland, Haporanda, below the mean, 1-3, 9, 15, 17-31, above the mean, 4-8, 10-14, 16, lowest on the 31st 5°, highest on the 4th 46°.

M. BUYSMAN.

MIDDLEBURG, Holland.

ROYAL METEOROLOGICAL SOCIETY.—The opening meeting of the session was held on Wednesday evening the 18th of November, at the Institution of Civil Engineers, 25 Great George Street.

Mr. R. H. Scott, F. R. S., President, in the Chair; Messrs. T. K. H. Clunn, R. S. Davis, B. A., H. C. Fox, M. R. C. S., W. E. Jackson, J. Richardson, M. Inst. C. E., F. G. S., A. L. Rotch, and C. Todd, C. M. G., were elected Fellows of the Society.

The following papers were read:

(1) "The Helm Wind of August 19th, 1885," by William Marriott, F. R. Met. Soc. This wind is peculiar to the Cross Fell range, Cumberland, and is quite local but very destructive. The chief features of the phenomenon are the following: On certain occasions when the wind is from some easterly point the Helm

suddenly forms. At first a heavy bank of cloud rests along the Cross Felt range, at times reaching some distance down the western slopes, and at others hovering about the summit—then at a distance of one or two miles from the foot of the Fell there appears a roll of cloud suspended in mid-air and parallel with the Helm Cloud; this is the Helm Bar. A cold wind rushes down the sides of the Fell and blows violently till it reaches a spot nearly underneath the Helm Bar where it suddenly ceases. The space between the Helm Cloud and the Bar is usually quite clear, the sky being visible; at times, however, small portions of thin vaporous clouds are seen travelling from the Helm Cloud to the Bar. The Bar does not appear to extend further West than the river Eden. The author visited the district in August last, and was fortunate enough to witness a slight Helm. He gives a detailed account of what he experienced and also his observations on the temperature of the air at the summit and base of Cross Fell, the direction and force of the wind, the movement of the clouds, etc.

(2) "The Typhoon origin of the Weather over the British Isles during the second half of October, 1882," by Henry Harries. The author shows by means of daily charts, that a Typhoon which originated near the Philippine Islands on September 27th, passed over Japan and the Aleutian Archipelago, entering the United States on October 10th. Crossing the Rocky Mountain Range it proceeded through the Northern States and Canada to Labrador and Davis Strait. In the Atlantic it was joined on the 18th by another disturbance which had come up from the Atlantic Tropics, the junction of the two being followed by a cessation of progressive movement from the 19th to the 25th. During this period the severe gale which passed along our southern counties on the morning of the 24th was formed, its sudden arrival upsetting the Meteorological Office forecasts of the previous night. Observations are quoted showing that it would have been impossible for the Department to have been aware of its existence before about 3 A. M., of the 24th. Following on the wake of this storm the parent cyclone reached the French coast on the 27th, its advent being marked as in Japan and America by violent gales and extensive floods over the whole of Western and Central Europe and Algeria,

The village of Grindelwald was destroyed, and in the Austrian Tyrol the damage caused by floods reached at least two millions sterling. Passing through France and the Netherlands the disturbance showed signs of exhaustion and on November 1st, in the Baltic, it quietly dispersed, after accomplishing a journey of over 16,000 miles in 26 days. This is the first storm which has been followed day by day from the Pacific to Europe.

(3) "Notes as to the principle and working of Jordan's Photographic Sunshine Recorder," by J. B. Jordan and F. Gaster, F. R. Met. Soc. This instrument consists of a cylindrical dark chamber on the inside of which is placed a prepared slip of photographic paper. The direct ray of sunlight being admitted into this chamber by small apertures in the side, is received on the sensitized paper and travelling over it by reason of the earth's rotation leaves a distinct trace of chemical action whenever the light is of sufficient intensity to show a definite shadow on a sun-dial. The cylinder is mounted on a stand with adjustments for latitude, etc. The record is fixed by simply immersing it in water for a few minutes. As this instrument records the actinic or chemical rays it usually shows more sunshine than is obtained by the ordinary "burning" sunshine recorder.

SANITARY SCIENCE AND METEOROLOGY.—Many facts developed in the last quarter of a century show how close a relation sanitary science bears to meteorology. Investigations by such men as Prof. Tyndall, as a physicist, and Pasteur, as a microscopist and pathologist, have proven that the salubrity of a locality does not alone depend upon conditions of relative or absolute humidity, atmospheric pressure, temperature or rainfall, although they may have important relations to it. We now know that the purity or impurity of the atmosphere must be taken into consideration. The contrasts of the death-rates of two contiguous communities like those of Pullman and Chicago, present an illustration of the relative influences of atmospheres approximately pure and impure. In Pullman, owing to the marvelously perfect system of drainage there existing, the annual death-rate is but 7 in 1,000 inhabitants, one of the lowest in the world, a fact that its climatic conditions in contradistinction with the climatic conditions per-

taining to Chicago, where the death-rate is much larger, will not account for. So in studying the relations of climatic influences to cholera, one cannot ignore the relations that atmospheric impurity has to its development. In a report by Dr. W. T. Van Verdenburg of New York city, in regard to cholera in Spain, he shows that the inhabitants in nearly all the towns and cities of that country depend for drainage and sewerage upon the drains and sewers built during the Moorish possession.

M. FAYE'S THEORIES OF STORMS.—Apropos of the interpellation of M. Mascait in the French Academy of Sciences, on his theories, M. Faye took occasion to repeat them in a succinct form. As the opinions of so eminent a student are worthy of being thoroughly and generally understood, we give his account of them in his own words, as far as possible. As to gyratory storms, he says:

1. *Trombes* (waterspouts, etc.) do not pump water from the sea, but, on the contrary, hollow its surface.

2. *Trombes* and tornadoes descend from the skies. This is not an illusion, but a reality.

3. The great gyrations have their seat, cause, and origin in the upper regions of the atmosphere.

4. The gyration of cyclones is mechanically connected with their translation.

The author speaks of "pretended" anti-cyclones, and says they have nothing to do with gyration. He goes on to sum up the principles of the new dynamical Meteorology.

1. Cyclones, typhoons, pamperos, *travades*, tornadoes, or *trombes*, as well as tempests, hurricanes, squalls, and storms, which all travel with great velocity in the ærial ocean and are associated with a sharp depression of the barometer, are formed in the great superior currents of the atmosphere, just as the whirlpools are formed in our rivers, follow the current of water and reach down sometimes to the bottom, washing it up in their progress.

2. There is nothing tumultuous in these winds: whatever their dimensions, they are regular, persistent, with the axis descending vertically. They can last, while travelling at a great velocity, (the reduced velocity of the current where they are formed and fed at the expense of inequalities of velocity in the said cur-

rent) either some hours only or entire weeks. Their translation is not at all modified, generally, by obstacles of the ground. The energy of their descent is measured by that of their gyration.

3. The upper currents leave the higher regions of the atmosphere in the region of the thermal equator, and flow toward the poles with an accelerated velocity, describing on the sphere parabolic curves with concavity eastward. These trajectories are symmetrical with respect to the equator in the two hemispheres. It results that the gyration of cyclones is direct in our hemisphere, and retrograde in the southern. The currents descend, as also their cirrus clouds, in proportion as the equator is left; the gyrations formed from them, sharply defined towards the tropics, are enlarged, weakened, and deformed more and more in the temperate regions, and disappear in the polar countries.

4. The rotations can, by enlarging, divide and produce distinct whirls of the same form *marchant de converse.* Inversely, whirls sprung from the same funnel, and travelling together, can reunite into one and add their respective gyrations.

Parasitic gyrations can originate in the immense spire of a cyclone, and give birth to great series of *trombes,* tornadoes, and storms, in the dangerous semicircle, a little in advance.

6. The mechanical effects of these whirls are always and everywhere the same. When they descend and meet the obstacle, the soil or sea, they expend upon this obstacle the *vis viva* collected above in a vast funnel, and concentrated below in a much less space; they tear up the soil or the sea, they tear them up like a plow which travels rapidly in a straight line, this while rotating almost horizontally and with great violence.

7. The physical effects depend on the constitution of the ærial river, more or less elevated, in the bosom of which the whirls took their origin and from which their gyrations are fed.

If the superior current is deprived of aqueous particles more or less congealed, the gyratory movement brings down dry air, warmed by compression; hence the phenomena of the fœhn, sirocco, etc. If also the gyration is energetic enough (and in this case the gyratory motion is propagated to the ground as a *trombe*) the over-heated air, having left the foot of the trombe on contact with the ground, will possess a certain ascensional force. It will

carry up torrents of sand and dust, collected horizontally from a distance and from all directions. It becomes visible by the dust it takes up in its progress.

8. If the ærial river contains aqueous particles, especially needles of ice at a very low temperature, the gyratory spirals will be cold in spite of the increasing compression which they undergo in their descent. They will then produce in the warm and humid layers of the lower atmosphere, clouds, showers, hail, thunder. If one of these gyrations descends to the ground, across the layer of nimbus, without breaking up its spirals, it will have arouud it a slightly conical sheath of condensed vapor which will render it wholly or partially visible. Finally the cold air, leaving the *trombe* tangentially, on contact with the ground, will not possess in itself an ascensional tendency, as in the case of the dry *trombes.*

These points are translated, almost word for word from M. Faye's remarks. We have not attempted to give a translation of *Trombe.* It is already used in English, and it is used by the author in this case in a sense different from waterspout (the definition by Bellows) while it is evidently not used in the extended sense given it by Peltier.

POTASSIUM IODIDE IN OZONOSCOPES.

This salt continues to be the chief essential agent in the variously modified tests for ozone, or for the excess of oxidizing activity represented by ozone in the atmosphere. Five or more ozonoscopes consist of iodide of potassium with different proportions of starch, in one it is used with litmus, and in one it is used alone. Other tests, as the use of thallous oxide, guaiacum, silver foil, and spectroscopic observation, have their several claims, but for stated observations of the atmosphere nothing obtains credit as a surer constant than a potassium iodide test, whatever may be the chemical uncertainties of its interpretation.* The

* Readers of the chemical literature of ozone find great convenience in the use of Prof. Leed's Indexes to the Literature of Ozone and Peroxide of Hydrogen, with historical summaries, 1880 and 1883: Annals New York Acad. of Sciences, i. 363, iii. 137.

value of any potassium iodide test depends on chemical qualities of extreme sensitiveness, co-existing with stability of the salt, and it is indispensable, first that its quality be uniform, and next that it be as near real purity as possible.

Absolute purity is a theoretical state, and its assertion brings in question, with what minuteness of measurement can deviations from purity be measured. At all events the attainment of uniformity is greatly favored when all ozonoscopes used by a corps of observers are prepared together and distributed from a single office, year after year; and independent observers will do well to avail themselves of the test materials so provided.

The writer has no experience in application of tests for atmospheric ozone, but has some experience with iodide of potassium and its imperfections. As abundant as "chemically pure" grades of this salt are, an article which does not give immediate indication of impurity under ordinary chemical tests, is very rare. Of impurities (1) an alkaline reaction, from presence of carbonate, or less often, hydroxide, is frequently found in the "C. P." article. The medicinal article is always alkaline, and the pharmacopœial limit of alkalinity is hardly regarded, five representative samples giving, in 1883, respectively 0.74, 1.84, 0.05, 0.24, 0.04 per cent. of alkali, as carbonate. Alkalinity in proportion to its extent diminishes the sensitiveness of iodide of potassium and starch tests and of tests of iodide of potassium alone, while an approach toward alkalinity might be said to increase the delicacy of the test with iodide of potassium and litmus. Now the test for alkalinity of iodide to be used in an ozonoscope should be made with an exactness of the final chemical equilibrium, suited to a measurement of those faint chemical influences to be revealed by twelve-hour tests of the atmosphere. Then (2) regarding presence of iodate of potassium. This compound is a final result of the action of ozone, peroxide of hydrogen, and numerous other oxidizing agents, upon potassium iodide. Traces of iodate, with iodide of potassium, cause a liberation of iodine on the slightest acidulation. Therefore when containing traces of iodate, a decided alkaline reaction is needful to prevent discoloration in the air. The test for free iodine, given in Dr. Fox's ozone, p. 184, with addition of acetic acid to very faint acidity, is in effect a test for iodate. Carbonic acid liberates

iodine and developes an alkaline reaction in iodide of potassium with a trace of iodate. It has been stated that pure iodide of potassium is acted upon by carbonic acid, but this probably lacks confirmation. Along with the iodate, revealed by the same tests and of similar effect in ozonoscopes, may be counted any other oxysalt of iodine, such as the hypoiodite, which may be present as an impurity in iodide of potassium. As already mentioned, iodate is a later product of the special oxidizing agencies of the air in action with potassium iodide, therefore this salt may acquire traces of iodate by storage. Now the tests for traces of iodate require careful estimation of the conditions of mass (concentration), temperature and time, and it may be suggested that parallel tests, for comparison with a salt of known purity, are desirable.

Chloride and bromide (3) are very hard to completely eliminate in making iodide of potassium. The chloride is so deliquescent that a very little of it has an appreciable hygroscopic influence on an ozonoscope; and both chloride and bromide undoubtedly directly modify the reactions liberating iodine, though they interfere much less than the impurities previously noticed. Both chlorine and bromine are carried in the iodine out of which iodides are made, their removal is not easy, and their detection in traces is quite difficult.

An exact experimental investigation into the effects of subtile impurities in the potassium iodide of ozonoscopes, the most favorable limits of variation, and the most efficient tests to fix these limits, would promise to be helpful to the important interests of meteorology.

A. B. Prescott.

Ann Arbor, Mich.

THE ARAGO-DAVY ACTINOMETER.

[CONTINUED FROM PAGE 354.]

In this actinometer, as has been stated, the difference at any time between the readings of the black and bright-bulb thermometers is taken as a relative measure of the intensity of solar radiation. At sunset this difference vanishes and does not appear again until sunrise, as should be the case if no heat is received from any

other source than the sun. But where, then, is the temperature of space, or, in other words, the heat received from the stars, the amount of which, according to Pouillet and others, is but little less than that received from the sun? If this is of the same nature as the sun's heat, as Pouillet supposed, and so can pass through our atmosphere and through glass with the same facility, the difference between the temperatures of the black and bright bulbs at midnight should be nearly one-third of the mean difference during the day in our latitude; for if nearly one-half of the whole heat received by the earth is from the stars, then the rate with which the earth receives heat from half the stars during a clear night should be nearly one-third of the mean rate with which it receives it from the sun and half the stars during a clear day, and so the difference between the readings of the two thermometers at night should be nearly one-third of the mean difference during the day. This mean difference is about 16° (29° F.) and so at night there should still be a difference of about 5°; but no such difference is observed, nor is there any difference sensible to ordinary observation. If there is a large proportion of heat received from the stars, this can be explained upon the hypothesis only of its not being like that of the sun, but of the nature of dark heat, which does not freely penetrate the atmosphere and the glass inclosure of the thermometers. But the stars are self-luminous and doubtless of very high temperatures, just as our sun is, and therefore, most probably, their heat is of the same nature as that of the sun, as Pouillet reasoned.

The most plausible way of getting clear of the difficulty above is to assume that the heat received by the earth from the stars is a very inconsiderable part of the whole heat received from the sun and stars, and that a sensible temperature of space is a fiction. This is in accordance with the views of Sir John Herschel on this subject, who assumed that the heat received from the sun is to that received from the stars in about the same ratio as the light received from the sun is to that received from the stars, and the latter he put at about 100,000,000 to 1. The mere ratio which he has given between the almost infinitely small proportion of heat received from the sun and stars may be considerably in error, but so far as the absolute amount of heat received from the latter is

concerned, the error, doubtless, is very small. If we even make the ratio above a million times less and put it as 100 to 1, the effect of the heat of the stars on the actinometer would scarcely be measurable, since it would be less than 0.1° in the difference of the readings of the two thermometers.

Assuming that the heat of the stars may be neglected, the actinometer then measures the intensity of solar radiation alone as received at the place of observation. But although this intensity is a function of the difference between the temperatures of the two thermometers, such that the one increases or decreases with the other and both vanish together, yet the latter is not a true relative measure of the former, for the variations in both are not proportional, and so the measure and the thing measured are not proportional, which is necessary to make it a true relative measure. It has been shown in a recent communication, (this Journal p 303) that, by the law of Dulong and Petit and the fundamental condition determining temperature, the difference between the temperature of the black bulb in vacuo and that of the glass inclosure, is greater in proportion to the intensity of solar radiation for large than for small differences, with the same temperature of the inclosures, and that the same difference is greater in proportion to the radiation, for low temperatures of the inclosure than for high ones. Exactly the same is true in the case of the bright-bulb thermometer, and this could be used just as the black-bulb thermometer is, in obtaining a relative measure of the intensity of the solar radiation, and the only disadvantage would be that the range of temperature variation assumed as a relative measure would be much smaller. If we, therefore, put t for the temperature of the black bulb and t' for that of the bright bulb, instead of the temperature of the inclosure, as in the case of the solar thermometers in the communication just referred to above, then $t—t'$ is greater in proportion to the intensity of solar radiation for small than for large values of $t—t'$ with the same temperature of the enclosure, or air temperature, and the same value of $t—t'$ is greater in proportion to the solar radiation for low than for high temperatures of the inclosures. Hence $t—t'$ is not a true relative measure, since it is not proportional to the thing measured.

For a temperature of the glass inclosure of 0°C. the value of $t—t'$, by Dulong and Petit's law, is about one-seventh greater, for the same intensity of solar radiation, than it is when the temperature of the inclosure is 20° higher; and hence, to be comparable with the latter, one-seventh part must be deducted. And so in general, using $t—t'$ as a relative measure, the same intensity of solar radiation measures more in winter than in summer. It was from the use of this erroneous measure that the unusually large value of the diathermancy constant p was obtained which is now used at the Montsouris observatory. This constant is the proportion of the heat of a vertical sun which passes through the atmosphere to the earth's surface. It was found to be 0.875, whereas the value usually found is from 0.75 to 0.78. By a reference to the Montsouris Annuaire for 1875, p. 59, it is seen that this value was obtained by taking the average of five values, obtained from as many sets of two equations of condition, in all of which a spring or summer observation of $t—t'$ in the one equation is used with a winter observation, Jan. 27, of $t—t'$, which is used in each of the sets of two equations. Now if we suppose that the air temperature of the winter observation was 20° lower on the average than those of the spring and summer observations, which is a very reasonable supposition, then, from what is stated above, it would be necessary to deduct one-seventh part from this winter observation of $t-t'$, in the equations in which it occurs, to make it comparable with the others, and solving the several sets of equations with this correction, we get five values, the average of which is only 0.769, instead of 0.875, and this is about the usual value obtained.

Although $t—t'$ is not a true relative measure of solar radiations, yet the true measure can be readily obtained from observed values of t and t' by means of a small table, just as the vapor tension of the air can be determined for any given altitude, from observed values of dry and wet-bulb temperatures; but $t—t'$ can no more be used as a measure of the intensity of solar radiation, because the one increases or decreases with the other and both vanish together, than the difference between the temperatures of the dry and wet-bulb thermometers can be used as a relative measure of the dryness of the air.

Having a true *relative* measure of the intensity of solar radiation, of course only some constant factor is needed in order to have an absolute measure in heat units of any kind per unit of time, minute or second. The table therefore referred to above could be made out just as conveniently for absolute as for relative measures. But on account of the difficulty of constructing thermometers which all give exactly the same indications, especially in the case of bright-bulb thermometers, it would be necessary to have a table of corrections to reduce the thermometer readings to some standard, or else to have a small separate table, such as referred to above, for each pair, to facilitate the obtaining of the true absolute intensity.

On account of the uncertainty in the exact proportion of solar heat passing through the thin glass inclosure, and in the amount of heat radiated by the thermometers which is reflected back by the inclosures, as well as some little uncertainty in Pouillet's determination of the rate with which heat is so radiated from each square centimeter of a lampblack surface, it may not be possible to determine very accurately the constant factor above. The amount of heat cut off by the glass does not differ much from that reflected back by the glass inclosure. By assuming them to be equal and putting the rate with which a square centimeter of lampblack surface at a temperature of 0°C radiates heat at 1.146 calories per minute, as determined by Pouillet, a constant can be determined from the relations of the temperatures of the two thermometers; and in this way we get intensities which seem to agree very well with those usually obtained by other methods. To obtain in this way the necessary constant with sufficient accuracy would require nice experiments to be made in order to determine the proportion of the sun's heat which passes through the glass inclosures, and the proportion of dark heat which is reflected back by the inclosures; and the experiments of Dulong and Petit should also be repeated with a radiating body with a surface of maximum radiating power, and also with surfaces of other radiating powers, instead of a glass surface, as used by them. It would also be desirable to have experiments on the rate of cooling in inclosures having greater or less approximations to perfect vacuums, since very small residuals of air left seem to have, in some instances, a

considerable effect. Any one having laboratory facilities might make many useful and interesting experiments of this sort having an important bearing on this subject.

But if the necessary constant cannot be obtained with sufficient accuracy in this way, all we have to do is to adopt such a constant as will make the indications of this actinometer agree with those of any other, or with the intensities of solar radiation as determined by any of the methods of calorimetry, if in this way it can be obtained more accurately. But actinometers so far, as used, have given results which differ very much, the range of variation being at least one-third of the average of all. But some certain method can surely be devised by which the rate with which the sun's heat is received can be determined; and if so, the rates obtained in this way can be used, not only for determining the constant of each actinometer, but likewise as a standard of comparison, where very accurate results are required. The actinometer, then, will have the same relation to this standard which the mercurial thermometer has to the air thermometer; so that, as the former is used for all ordinary purposes, on account of its convenience, instead of the latter, but the readings are corrected so as to agree with those of the latter, where great accuracy is required, so this actinometer, on account of its convenience, both of transportation, and of obtaining the necessary indications in a few minutes at all times and in all places, can be used instead of any cumbersome apparatus, inconvenient to transport and to manipulate, although more accurate and reliable, and such small corrections can be applied as will reduce its indications to those of the latter. The Arago-Davy actinometer, used in this way, will be a very convenient and accurate instrument for obtaining, not only the relative intensities of solar radiation, but likewise the absolute intensities in heat units per unit of time, whatever the assumed units may be, and must become of general use.

WM. FERREL.

ON THE DETERMINATION OF THE TRUE AIR TEMPERATURE.[1]

It is very gratifying that the attention of meteorologists at the present time is specially directed to the important question of the determination of air temperature, and it is still more gratifying that two scientists in lower latitudes, Director Mielberg in Tiflis (lat. 42°) and Prof. Hazen in Washington (lat. 39°), where disturbing influences are felt in a higher degree, have attempted the solution of this question. Mr. Mielberg in these investigations uses my method of sheltering thermometers, in order to ascertain its efficacy in eliminating disturbing influences of radiation, while Mr. Hazen gives the preference to the elimination of radiation by rapid motion of the thermometer, *i. e.* to the sling thermometer.

We learn from the communication of Mr. Hazen in the March number of this journal [Z. M.] (extract from an article by the same in the January and February numbers of the Am. Met. Journal) that he was not satisfied, any more than his predecessors, in using the sling thermometer as giving the air temperature more correctly than other methods, but considered it imperative to look for an experimental proof of its accuracy, in a similar manner as I have done in the case of my thermometer shelter. Since the allegation of this supposed proof in a very condensed form is followed by a much more elaborate criticism of my shelter by Mr. Hazen, partly with reference to my article in the Oct. 1884 number of this journal, I, on my part, may be allowed to throw some critical light upon his method and to add a few words in reply to his censure of mine.

Prof. Hazen has had the happy thought to join to the common bright bulb thermometer another with blackened bulb, and to use these as a sling system. This represents a combination of Arago's principle of the simple sling thermometer (properly speaking,

[1] The translation we give herewith is that of a paper by Professor H. Wild, Ph. D., of St. Petersburg, published in the *Zeitschrift fur Meteorologie*, May, 1885, pp. 161–175. So much has been said about this subject that we are sure our readers will be glad to have this paper made accessible to them. This translation has been submitted to Prof. Hazen and he has appended some notes. The brackets are interpolations by Mr. Hayzen.—Ed.

Saussure had long before commented upon the difference between the indications of a sling and fixed thermometer) for the elimination of disturbing radiations, with Liais' proposition to determine the influence of the latter by simultaneous readings of three thermometers, having their bulbs similarly exposed and covered with different radiating substances (Comptes Rendus 1851, p. 207).

Mr. Hazen remarks in his introduction, very correctly, that the radiations with which the meteorologist deals are divisible into radiation of the sun, which he subdivides again into direct and reflected radiation of the sun, and into dark radiation or radiation of non-incandescent bodies; among the latter he emphasizes especially radiation into space. Unfortunately Mr. Hazen himself has not strictly adhered to this division as we shall see in the course of our discussion[1]]

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[1]Notwithstanding my earnest endeavor to make clear the different classes of radiation and to establish a common ground for clearly distinguishing and weighing them, Prof. Wild, here, and in about half of his remaining criticisms, has persistently misapprehended what I tried to make clear beyond doubt, and has entirely misunderstood, as it seems to me, the relative importance of these radiations. Without a clear understanding of these radiations and their relative importance, we shall labor under great uncertainties and our discussions will become hopelessly involved in confusion. I may, therefore, be permitted to repeat and amplify what I actually wrote. As the article to which I shall refer received consecutive paging in this journal for January and February, 1885, I shall take the liberty to refer to it by page only. On page 342 we find "The radiations the meteorologist deals with may be divided into four general classes: 1st, Solar radiation; 2nd, Reflected heat, the same in kind as the 1st; 3rd, Dark radiation or that from bodies not incandescent, and 4th, Radiation into space." The importance of the third of these as affecting the thermometer directly has been much exaggerated. It will be admitted, I think, that the direct effect upon a thermometer bulb of dark radiation from objects a few feet away, is very slight; it is very different, however, when we come to consider the effect of this radiation upon the air immediately surrounding the objects giving off heat, as in this case the air we are measuring receives heat from its surroundings, or parts with its heat to the same, and we are driven to measure simply the air temperature as affected by the above conditions. We certainly can never go farther than to measure the temperature of the air immediately surrounding the thermometer, after shielding the latter from as many harmful radiations as possible. The

Mr. Hazen maintains that the true air temperature t_a may be generally computed from the readings t_b of the bright bulb and t_s of the black bulb thermometers, by the formula

$$t_a = t_b - c(t_s - t_b),$$

where c is a constant nearly equal to unity. Eight observations made on an open lawn at different times in November, 1884, with three different exposures of the two thermometers, namely, 1. under an umbrella in full sunshine, 2. directly in full sunshine, 3. in the shade of a little barn[1]], gave, with elimination of the temperature variation during the time of observation, with still and sling thermometers, the following results in degrees C:

Under Umbrella.				Full Sunshine.				Shade.			
Still.		Slung.		Still.		Slung.		Still.		Slung.	
t_s	t_b	t_s	t_b	t_s	t_b	t_s	t_b	t_s	t_b	t_s	t_b
7.17	6.78	6.72	6.44	10.39	8.00	7.33	6.67	6.28	6.00	5.94	5.83
$t_s - t_b = 0.39$		0.28		2.39		0.66		0.28		0.11	
$t_a = 6.39$		6.16		5.61		6.01		5.72		5.72	

In the last line the values for t_a are computed from the above formula by assuming $c = 1$. Mr. Hazen adds to the above the following reflection: "It seems probable that the results for 'Umbrella slung' come nearest to the true air temperature. If we suppose in this case that $c = 1$ we may determine values of c for the other cases, *e.* g. umbrella, still, $c = 1.59$; sun, still, $c = .77$;

effect of the 4th radiation above has been very much misapprehended. If any one will suspend a thermometer in the open air, on a clear day and in the shade, he will find the effect of radiation into space from the bulb almost inappreciable and, moreover, the amount of this effect will be precisely the same, no matter whether in the shade or sun. Far otherwise, however, will be the effect on the air of this radiation into space, when we compare the radiations from a shaded spot (I mean shaded, not overhead, but by a barn or wall) with that from a spot on which the sun is shining. In the shaded spot the ground or sod will be much cooler than the air and will continually absorb its heat, while, in the sun, the ground will be hotter than the air and will have a tendency to heat it.—H. A. H.

[1]] Prof. Wild translates this indifferently "little barn" "hut" and "*little hut*," it was 26 ft. high and 30 ft. long. The size of the shadow at midday in November was about 100 ft. long and 30 ft. wide.

sun, swung, $c = .77$. *In the shade (of the hut) the effects of solar radiation [on the air] are entirely cut off though reflected heat is very slightly felt [by the thermometer and the air]; at the same time the effects of radiation into space [from the soil] are at a maximum, and in consequence the temperature [of the air] obtained is about .44 too low.* This investigation, while not absolutely confirming the value of c as unity, with the black and bright bulb 'fronde' under an umbrella in full sunshine, yet seems to show that this method will give a better air temperature than any yet devised."

With best intention, I cannot justify these conclusions, and I believe that any one unprejudiced will agree with my view. His investigation gives no information whatever as to how far this method might determine the true air temperature, in fact, it seems to show merely that the formula given by him for computing the t_a from t_b and t_s slung is entirely unsatisfactory. With this in view let us consider his data more closely.

Mr. Hazen has not computed the value of c in the shade of the little barn. Why this was omitted he does not state; the passage in italics seems to indicate that he did not consider the formula applicable in this anomalous case. However, it seems to me it can be used here if in other cases. All the elements active in the second case viz., solar radiation, and a small part of reflected heat as well as the greater part of radiation into space [Prof. Wild means from the thermometer] are eliminated in the first case, under the umbrella, whereas the radiation from the umbrella [to the thermometer] is additional and that from the shady part of the soil [to the thermometer] is modified. (The two latter elements Mr. Hazen seems to consider unessential) [1]]

In the shade of the hut, direct solar radiation, a larger part of the reflected heat, and a smaller one of the radiation toward space, and a portion of the diffuse heat of the soil, are eliminated,

[1]] Prof. Wild has failed to see that to protect from the sun in November an umbrella must be held with its handle nearly horizontal, so that the radiation from the thermometer into space would not be interfered with at all. The umbrella was also held so far from the observer that its radiation to the thermometer was *nil*. Finally no effect from the soil was appreciable, from the fact that in making the observation the observer walked over different parts of the field.

whilst the radiation from the hut and a modification of the radiation from the shaded part of the soil [on the thermometer] are additional. If we apply the formula in the latter case we obtain, in shade of hut, still, $c = -.57$; slung, $c = -3.00$. But a quantity which differs from 1.59 to — 3.00 is neither customarily, nor in meteorology, designated as a constant. This proves either that c is not a constant or that the formula is defective. But there is another reason for which I have had the passage marked. Mr. Hazen says the observation in the shade of the hut (computing $c = 1$) had shown a temperature .44° too low, because solar radiation was entirely, reflected heat almost entirely, excluded, but that in compensation the radiation into space was at its maximum. But if the radiation in this case had indeed so exceeded the absorption, that the thermometers show a temperature nearly .5° lower than that of the air, then evidently the temperature ought not to have fallen .17° more, but it ought to have risen because of the motion which, as we know, tends to lessen the effect of radiation, especially with the bright bulb thermometer, which showed, when still, a temperature .16 lower than the supposed air temperature. The observations in the shade of the hut, if we accept the suppositions of Mr. Hazen, admit only one conclusion, that the true air temperature had been at the utmost 5.83 instead of 6.16. Only by adding, besides the elements of radiation suggested by Mr. Hazen, a new source of refrigeration which becomes specially effective during the swinging of the thermometers, or a source of heat which is eliminated during this operation, can the results in the shade of the hut be made consistent with a higher air temperature than 5.83. A source of heat, in the latter sense, is perhaps represented in the person of the observer.[1]]

[[1] Prof. Wild has here overlooked what seems to me to be the most simple explanation of the higher reading in the shade of the still thermometer, as compared with the one in motion. His explanation is entirely untenable and, in fact, if properly understood, proves exactly the opposite of what he intends. The effect of heat radiated direct from the person to the thermometer is absolutely inappreciable, if proper precautions be taken, but, even though it were appreciable, in the case before us the still thermometer had *no* person near it, while the one in motion was necessarily near the slinger, so that, from this cause, the slung thermometer ought to have been the higher. The real reason for

If now we consider merely the temperatures observed directly by Mr. Hazen in connection with existing circumstances, it seems that in full sunshine, where, after slinging, the difference between the thermometers is still .66, the overbearing effect of solar radiation has not been eliminated, that hence the lowest temperature obtained there [6.67] was at all events higher than the true air temperature. The umbrella excludes direct solar radiation, and removes a small part of the reflected heat, but it eliminates also the larger part of the radiation into space, adding heat radiated from the umbrella [On these last two points see previous note. H. A. H.], so that the radiations here are even more than in the sun. It is doubtful, therefore, whether the influence of the former is weakened enough through slinging, so that the temperature 6.44 is not much higher than the true air temperature. That this is the case is shown by the observations in the shade of the little barn, where, in spite of diminished solar radiation and the increased radiation [from the thermometer], the slinging still seems to diminish the temperature so that the true air temperature could not have exceeded 5.83. The experiments of Mr. Hazen then give an uncertain maximum value to the air temperature. This method of determining the true air temperature, which he considers so certain has really accomplished much less than he claims, as shown by the experiments. Mr. Hazen obtains a value of some kind for the air temperature, only by the assumption that *c*, in a doubtful formula, is at times equal to 1, but he furnishes no proof for the correctness of this assumption. [1]] * * * * (Prof. Wild's discussion of a theoretical value for *c* is omitted.)

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this apparent anomaly is this: the reflected heat from surroundings directly raises the reading of the still thermometer, but this is overcome by the slinging.—H. A. H.

[[1] In all of this discussion Prof. Wild has utterly misunderstood my position and, moreover, has failed to comprehend the real situation of affairs. His greatest mistake is in the interpretation of the sentence which he has printed in large type and which is here italicized. Professor Wild surprisingly fails to see that there are two distinct air temperatures; that of the air in the sun, and that of the air in the shade of the barn. It seems to me absolutely certain that with no wind the air

A solution of the problem of determining the true air temperature I have tried to find, and believe I have found, in another way. Prof. Hazen does not share this view, (I come now to the second part of my reply concerning his criticisms of my method,) and this is probably the reason why he only says of my observations in 1876 and 1878 published in "Repertorium für Meteorologie" Vol. IV, No. 9, which seemed to me to prove a too high reading of the unsheltered sling thermometer at noon, and too low a reading in the evening and at night; "Prof. Wild regards this as giving too low value at night and too high in the daytime. He claims that, since a free thermometer indicates too low a reading upon a clear night, therefore this device will do the same. It is well known that radiation into space is largely diminished upon a windy night, though clear, hence, since the thermometer is in rapid motion and is brought in contact with a large mass of air, we must conclude that radiation into space [from the thermometer] has little or no effect." This Mr. Hazen declares after having, in the same article fourteen lines above, from his own ex-

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through which the sun's rays were passing, and which in addition, was heated by convection from the warmish sod, was beyond a shadow of doubt about .5° warmer than air in the shade of the barn where there was no effect from direct solar radiation, and where also the rather cold sod, cold, not only from radiation into space but also from retaining the cold of the night, in fact almost frozen, kept the air above it cool. Hundreds of observations have absolutely proved that the above hypothesis is correct. Prof. Wild has also seriously erred in supposing that I could possibly have considered that radiation from the thermometer bulb, into space in the shade, had any influence at all. That I could not have meant this is plain from the following considerations. 1st. In daytime a thermometer freely suspended in the shade, is not appreciably affected by radiation into space; as a matter of fact, this effect is entirely overcome by reflected heat. 2nd. Even if there had been any effect from this cause it would have been removed by slinging. 3rd. If there had been any effect from this cause, it would have been the same in all three cases, umbrella, [which did not hide the sky in the least], full sunshine, or shade of barn, so that, as far as this radiation was concerned, the observations were directly comparable in all three cases. 4th. If there had been any effect of this kind, the thermometer would have read higher after slinging than before, whereas it read lower. Strangely enough Prof. Wild happened on this very idea, but uses it to criticize me for a supposed

periments, drawn the conclusion that over a sunny lawn, in the shade of a little hut[1] where reflected heat was still somewhat felt, yet, because of excessive radiation into space [from the sod] his sling thermometer has given a temperature which was .4 too low. To be sure, Mr. Hazen continues three lines below as follows: Prof. Wild's experiments with a sling thermometer, in deep shade and then in the sun, cannot be considered perfectly satisfactory; as already shown, such shade has a tendency to give far too low a reading."[2]] Mr. Hazen is of the opinion that, although, on a clear day

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oversight, which I certainly did not make, and explains the anomaly in a manner utterly untenable. 5th. The effect of radiation from the sod cooling that down, and in turn cooling, by convection, the air above it, in addition to the cooling due to the shutting off of the sun's heat, is all that I intended in my allusion to *radiation into space* and *solar radiation.* This is also shown as regards radiation into space a few lines below those quoted by Prof. Wild, near the bottom of (p. 344), where I say: "As already shown the dense [deep] shade of a building is objectionable [for obtaining air temperature] because of great radiation into space and a consequent fall in temperature [of the air] below the [general] air temperature [or that of the surrounding region]." I do not think I need to go over Prof. Wild's criticisms point by point, as the above explanations cover the whole ground. Prof. Wild now seeks to find a theoretical value for c and finds it 9. This is entirely too large and I do not need to dwell upon it. Prof. Ferrel has made a study of the question and has determined a formula in which, if proper assumptions are made, the value of c comes out less than 1. (See Prof. Papers, Sig. Ser. No. XIII. pp. 60 & 61). From a long series of careful experiments I have determined the value of c as between .5 and .7, changing slightly with different conditions of insolation, ventilation, etc. After a most complete investigation of this whole question I can only assert again, that the method of black and bright bulb sling thermometers is calculated on all accounts to give an accurate value of the air temperature of any spot, and that no method yet proposed can compare with it in simplicity or accuracy.—H. A. H.

[1]"For sunny lawn in the shade of a little hut" read "deep shade of a barn 30 ft. long and 26 high."—H. A. H.

[[2]. Prof. Wild while ordinarily quoting from my paper in AM. JOURN. MET. has in this instance, very unfortunately, quoted from an abstract of the original, published in Zeitschrift für Meteorologie for Mar. 1885. If he had here noted the former (which I have already given under the 5th of my reasons why I could not have meant radiation from the ther-

in the shade of a little barn (he calls this deep shade), the motion of the thermometer is not able to eliminate the influence of radiation into space, but that, in consequence of this, its indication is still .4 too low, on the contrary, in the certainly deeper shade of a clear night, where the soil cooled by radiation forms a new source of cold, this motion must absolutely prevent a cooling of the thermometer by radiation into space amounting to .5 below the temperature of the air, as determined by my experiments.

This is evidently a remarkable interpretation of facts, but Mr. Hazen furnishes us with something far more astonishing, when he turns toward the further conclusion from my comparisons between the sling thermometer and my exposure, namely, that at midday, during bright days, the former without protection gives too high values. That the reader may judge for himself I give the table of comparisons.[1]]

June and July	Wild Shelter	Sling	Diff	Clouds 0-10
h	°	°		
19 15.00	20.3C.	21.5C.	-1.2	3
19 21.00	17.2	17.4	- .2	4
20 8.00	17.4	18.6	-1.2	3
24 7.00	15.5	16.4	-0.9	8
24 13.00	19.8	21.0	-1.2	5
27 13.00	15.0	16.8	-1.8	0
7 13.00	21.4	22.4	-1.0	3
May.				
15 15.00	20.8	21.2	- .4	0
15 15.30	20.6	20.8	- .2	0
15 16.30	21.0	20.8	+.2	0
15 18.00	20.5	20.1	+.4	0
15 19.00	17.6	17.4	+.2	0
15 20.00	15.9	15.7	+.2	0
15 21.00	14.4	14.0	+.4	0
21 14.30	14.4	14.7	-.3	5

To the table I added these remarks. "Since, according to the above [remarks that preceded table], our exposure indicates the

mometer bulb), he would have seen clearly that my whole and only reference was to the radiation from the soil, as affecting the air temperature above it, and he would have been saved the necessity of writing more than half his adverse criticisms.—H. A. H.

[1]] A comparison with the table on p. 401 of this Journal for Feb. 1885 will show that that is an exact copy of this, as far as vertical arrangement and essential figures are concerned.

true air temperature, the sling thermometer showed, during the higher position of the sun in June, a temperature throughout 1° too high. On May 15th, however, the sling thermometer showed at 16.00h a temperature equal to the air, in spite of the sunshine, and fell later below it, because, aside from radiation into space, that toward the cold soil caused a cooling. (At 16.00h a thermometer lying on the surface of the earth showed in the sun only 15°.)"[1]] * * *

According to Mr. Hazen, because the sling thermometer gives (nearly) absolute values in a partly cloudy sky [when solar radiation is nearly cut off], although the sky was in three cases entirely clear, (on the contrary, too the sun was always shining) the conclusion is that my shelter had given by day [by the table] temperatures 1° too low. But how does this harmonize with Mr. Hazen's other conclusion? "Mr. Wild's case gives necessarily during the day, (if clear) a temperature which is higher than the general air temperature."[2]]

This conclusion is based mainly, if we disregard vague or

[1]] I quoted this table because it was published by Prof. Wild, and seemed to have been selected from a large number of other observations. I did not quote it as settling the question one way or the other, but simply as showing that, in the first part of the table, with partly clouded sky, the sling thermometer always read quite high, while in the second part of the table, with clear sky, the reverse is true. That the plus signs of the 15th of May were not due to radiation into space from the sling thermometer, is shown by the fact that at 16.30h when, according to Prof. Wild, the air temperature was at a maximum and had risen .4° in the hour just preceding, consequently showing that the sun was hotter at this hour, yet the sling thermometer had not changed its reading in the hour; moreover, at 19.00h and 20.00h when the air temperature had fallen 3.4° and 5.1°, we may suppose, on account of the declining sun, the effect of radiation into space had not increased at all the difference between the two thermometers. Most careful experiments have shown that the effect of radiation into space, on a very clear night, cannot equal .1°C. and probably does not equal .05 at latitude 40°. If Prof. Wild will sling his thermometer at the level of those in his zinc screen, he will find, at least in latitude 40°, on the lee side of the shelter, on a clear night, the sling about .05° higher.—H. A. H.

[2]] It should be noted these experiments were made in lat. 60° and that the results are not strictly comparable with those at 40°.—H. A. H.

groundless assertions, upon the following three arguments: 1st. Lack of ventilation in my shelter. 2nd, Observations by Mr. Mielburg in Tiflis, in my shelter, and 3rd, The same by Mr. Hazen in Washington. As to the observations in Tiflis, I have shown that they question only the special construction of the outside wooden shelter, which was calculated only for higher latitudes.

Mr. Hazen mentions two kinds of observations with my shelter. 1st, he has found, as at Tiflis, from 9.00h to 15.00h on calm summer days [mean of a large number], in Washington, the temperature in it, without ventilation, .6°, with ventilation, .3° too high. He does not give the basis for his air temperature. If, as is probable, this is the sling thermometer, this objection has no weight as he still owes us the proof of the accuracy of the same.[1]]

During a series of observations in October 1883, Mr. Hazen found in his open shelter t_b=23.0: t_s=23.4, i. e. t_a= 22.6, c=1 Wild shelter t_b=23.8, t_s=23.9 i. e., t_a= 23.7. But we have concluded that with fixed thermometers c is by no means unity. But even if the value of t_a were correct and represented the air temperatures in both shelters, this does not by any means prove, as Mr. Hazen again very rashly concludes, that the one in my case is too high, the other may just as well be too low.[2]] * * * *

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[1]] The comparisons were made between Wild's shelter and an open shelter cutting off nearly all radiations, except from reflected heat, and allowing as perfect a natural ventilation as possible. The .6° is from the mean of 20 observations in all kinds of weather. I have found individual cases in which the Wild shelter was at least 2.6°C. too high.—H. A. H.

[2]] It seems to me that Prof. Wild has overlooked a very important point here, namely, that whatever value we adopt for c, since the effect of reflected heat in the open shelter must raise its temperature above that of the air, it can never give *too low* a temperature. It is difficult to see how any shelter allowing a free access of the air we wish to measure can give too low a reading in sunshine, when all the sources of heat radiation are most active; in fact, this (too high reading) is just the difficulty in all shelters in low latitudes. Again it is easy to see how reflected heat, (precisely the same in action as direct solar radiation), from the louvre work of the outside shelter, should tend to raise the temperature of the inside zinc screen. In addition, the lack of ventilation, which lack must under all circumstances be greater than that in the open shelter, would cause a stagnation of air, which would raise the tempera-

In the Stevenson screen, on account of its double louvres, shaped like a roof, [inverted v], its solid floor and roof, ventilation is very difficult and, moreover, since wood is a poor conductor of heat, the whole arrangement cannot be in equilibrium with the air temperature, especially at times of extremes. But double louvres are necessary, if all reflected heat is to be warded off. For these reasons, I thought it better to decrease the effects of radiation by a slightly polished inner metal case, which, by means of wide openings and because of its better conductivity and less specific heat, very readily comes into equilibrium with the air temperature, and to surround the same with a wooden case to protect merely against direct solar radiation. Since this wooden case stands on four posts, three metres above sod, has no bottom or north side, has louvres on the east and west, and the free air space in the double roof is connected with the free air space in the south side, no stagnation of air can occur. Only from the open louvres on the east and west is there any danger, as the editor of this journal has remarked very correctly on p. 431, Z. M. 1884. While Dr. Mielberg of Tiflis was in St. Petersburg in Aug. 1884, he said the heating of the walls of the wooden shelter to 59°C referred only to the outer walls, that he had never noticed a perceptible rise in temperature on the inner side of the south wall or on the ceiling, and he thought the source of error was mainly due to the air passing over these highly heated louvres to the tin screen inside. In order to avoid this in lower latitudes, having intense insolation, I have proposed making the east and west walls also double. When Mr. Hazen says of this "it would be difficult to find a less ventilated arrangement [for a thermometer shelter] except a closed box," he has evidently forgotten that this shelter stands entirely free 3m. above ground, and that it is entirely open underneath and to the north and resembles much more a *drawing chimney* [Zugcamin] than a closed box.[1]]

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ture in the zinc screen, and this much more, as compared with the open shelter, with a gentle south wind, when there would be no natural ventilation in Prof. Wild's shelter.—H. A. H.

[1]] It seems to me this last comparison of Prof. Wild's is very unfortunate. A chimney cannot draw unless the air in it or the chimney itself is heated. I still insist that in the above construction there would be no

The objection that the air carried into the zinc screen by the ventilator may have been heated by the wooden walls is raised by the editor in the note already mentioned and by Mr. Hazen. I think that here nothing more is to be feared than with any other method of exposure. The ventilator gets its air, not from the sides, but from below, and this air is much nearer the air temperature than that under Mr. Hazen's shelter. The heated air rises on the outside of the wooden case and only exceptionally will any of it be forced into the interior. In the case of the sling thermometer, heat from the observer who is, as we know, neither a diathermal [?] nor non-radiating body, will affect the readings. Besides in order to make the reading, the motion must be stopped, and the instrument drawn near the observer; while, with my shelter, the reading can be made with a telescope, the ventilation being kept up by an assistant.[1]]

The final objection to my shelter, urged by Mr. Hazen, as to the efficacy of ventilation, is one that I have already alluded to on p. 444, Z. M. 1884, but in another sense, and this is that my ventilator, as ordinarily set in motion, drove the air only against the sides of the screen. Now the object of this, as I wrote in my article, was to permit the use of the ordinary psychrometric tables, but,

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natural ventilation, but only the rising of currents hotter than the general air stratum about the shelter. When Prof. Wild says that double louvres are necessary to ward off reflected heat, he forgets, I think, that this very reflected heat is the same in quality as direct solar radiation, and that it is this that raises the temperature of his zinc screen, much more than the passage of heated air over the louvres; when we add to this the shutting off of all south wind, and the fact that the sides of the zinc screen present nearly a perfect obstacle to a free ventilation about the thermometers, we have no difficulty in understanding how there could be a temperature at least 2.6°C too high, with a gentle south wind, and an actual temperature, on the north wall of the zinc screen at least 4.1°C above the general air temperature.—H. A. H.

[1]] I think the action of this ventilator will draw in much more air from the heated sides than Prof. Wild allows for; as far as the use of the sling thermometer is concerned there is not the slightest danger of the heat of the person affecting the reading, as I have satisfied myself by hundreds of experiments. It is only necessary in a wind to stand to the leeward of the thermometer, and in a calm to walk a short distance, continually passing into fresh air.—H. A. H.

for the determination of the air temperature, the ventilator could be put in much more rapid rotation; above all, sufficient to make the walls of the case assume the air temperature, which is the main thing.[1]]

Since the unprotected sling thermometer, by the experiments of Mr. Hazen, not to mention mine, gives in sunshine too high, and by night [.05°C. H. A. H.] too low values and since, according to him, the shade of a house also gives incorrect values, we must even there resort to a shelter. Hence the question is: which is better, a sheltered sling thermometer or a fixed thermometer with ventilation.[2]]

As to the remarks of Mr. Hazen concerning the determination of the humidity of the air in general, as well as in my shelter specially, I will not waste a word here, since they neither refute

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[1]] Prof. Wild thinks that a change of less than .1° after ventilation is sure proof that his ventilated thermometer gives the true air temperature. I presume that this criterion may be fairly satisfactory in latitude 60°. I still think, however, that the actual passage of air by the thermometers is not great, even with rapid rotation. Again in lower latitudes, the fall in temperature after ventilation is on the average .3°C in the morning hours of summer days, and this fall would be much greater if the air were propelled more rapidly and were not drawn from the intensely heated walls, heated to an extreme because of the lack of ventilation. I have not tried the experiment, but I have little doubt but that the temperature indicated by a thermometer on the outside of the south solid wall of Prof. Wild's wooden shelter would be 10° to 15°C higher than on the south wall of my open shelter, and that this is entirely due to the lack of ventilation. It is astonishing how the slightest breeze will make the temperature fall, where there is a perfect access of air, while the thermometer in Prof. Wild's zinc screen will not be in the least affected.—H. A. H.

[2]] Prof. Wild, I think, goes too far in suggesting a shelter for the shade, the trouble is there is too much shelter there already, and no manner of shelter can possibly reproduce the temperature of the average air in sunshine. I have advocated all along as a normal air temperature, either that obtained with a sling thermometer, in the shade of a tree with not too dense foliage, or with a bright and black bulb sling arrangement anywhere, provided the actual air temperature is not itself abnormally affected there. This value, however, I would use only as a standard; for ordinary observations, I would use an open shelter allowing as free an access of the outside air as possible.—H. A. H.

anything I have before written nor add anything new to the question. The contents of this reply may be given as follows: The conclusions which Mr. Hazen has drawn in his article about the failures of my shelter, must, in their generality, at least, be designated as premature, just as his assertion, that the method of determining the real air temperature named by him, is more exact than any other used before, has proved to be throughout premature.[1]]

December 11, 1885.

SELECTED ARTICLES.

ATMOSPHERIC ELECTRICITY AT HIGH ALTITUDES.[2]

The following experiments were made at Blue Hill Observatory during the month of June, 1885. The summit of the Hill has an elevation of 635 feet above the sea level, and is therefore the highest point on this section of the Atlantic seaboard. With the exception of the two or three other hills in the range, all the surrounding country is very low and level. The average elevation is below 100 feet. On all sides this low land is well watered, having rivers of fair size and many ponds. For these reasons it was thought profitable to make some observations on the electrical state of the atmosphere, similar in nature to a series made for the United States Signal Service at the Jefferson Physical Laboratory in Cambridge.

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[1]] I did not in my paper present any comparisons of humidity between Wild's shelter and my open one, because it was difficult to make such comparisons owing to the extreme diversity of ventilation in the shelters and to the abnormally high temperature in the first. A long series of readings gave relative humidity in day time .6% lower in Wild's shelter, and at night time 2.3% higher in the same, than in the open shelter.

It is to be regretted that so much time has been spent in discussing matters of small consequence, to the neglect of those more weighty; at the same time, if from all this discussion we obtain a little clearer idea of the essentials of a proper exposure, and the causes of the defects in any one form, it will be of great and lasting advantage. H. A. H.

[2]]From the proceedings of the American Academy of Arts and Sciences. Communicated June, 1885.

The following apparatus was taken to the summit, and employed as hereafter described:—

A multiple quadrant electrometer, designed by Professor Trowbridge, a description of which may be found in the Proceedings of the American Academy, June, 1885.

A portable battery of 100 Beetz cells, set up in series.

A second battery of the same kind, the cells arranged for convenience in sets of ten.

A newly set up Daniell cell.

Two large light kites, silk-covered and tinfoiled on the front face; the longest axes of the kites being over 4 feet.

1,500 feet of strong hemp fish-line, around which in a close spiral was wound No. 22 uncovered copper wire.

50 feet of insulated office wire, and some 10 or 12 feet of rubber tubing, to better insulate the office wire.

One electrometer commutator, and some binding screws.

A condenser, with a capacity of ⅓ farad, was also brought up, but when required for use was found to be defective and of no value.

The first observations were made on June 17, at 9 A. M.

The first step was to measure the difference between the potential of the air, at a point a few feet out from the observatory walls and about five feet above the ground, and the ground potential. Instead of using the insulated water-dropper devised by Sir William Thompson, I made use of the method employed with success at Cambridge; namely, of dropping water on an insulated metallic plate, and allowing it to fall in drops therefrom. Making the electrometer connections in the way adopted by Thompson and English writers generally,—that is, connecting one set of quadrants with the insulated plate, the other set with the ground, and bringing the needle to a high potential by connecting with the plus pole of a battery of high electromotive force,—the electrometer indicated no appreciable difference between the potential of the air at that point and the potential of the ground. The ground, it may be remarked, was not at the observatory itself,—for the summit is a ledge of solid rock,—but a telephone ground, made of a wire running down the hillside some distance, and connected at different places with metallic plates buried in the earth.

Using the method adopted by Mascart in making the electrometer connections,—that is, connecting one set of quadrants to the plus pole of the 100-cell battery, and the other set of quadrants to the negative pole of the same battery, while the needle is connected with the insulated plate or body whose potential is to be determined,—no appreciable deflection due to the difference between the potentials of the air and ground could be noticed. The insulated plate has to be disconnected, and in its stead the ground substituted in order to ascertain this difference of potential. The electrometer is designed to measure only where differences of potential exist that are of considerable value. The needle of the electrometer carries a fine aluminium pointer, allowing one to read the deflections directly.

In the mean while, the kite having been raised to an elevation of 200 feet, the wired kite-string was now connected with one set of quadrants, and the other set connected with the ground. The needle was connected with the plus pole of the 100-cell Beetz battery, the other pole being grounded. Great care was taken to insulate very thoroughly the kite-string, as with electricity of high tension ordinary methods of insulation are not sufficient. Instantly on making connection, the needle was deflected with a considerable impulse beyond the limit of the scale (25 scale divisions), and until stopped by the side of the case of the instrument. The deflection indicated a very high positive potential for the air in the vicinity of the kite. To decrease the sensibility of the instrument, the battery charging the needle was reduced from 100 cells to 10 cells, and finally to a single cell. The Beetz battery of 100 cells has a difference of potential between its plus and minus poles of about 100 volts. The needle-pointer being at 0, the plus pole of the battery being connected to one set of quadrants, caused a deflection of 5.5 scale divisions. Connecting the insulated kite-string to the same terminal, the aluminium pointer was deflected with force to the side of the case. The least value of the difference between the kite potential and the ground was about 500 volts. The wind at this time was blowing freshly from the northwest, and the kite was seemingly stationary. Touching the ground wire, for a second, to the kite wire, a small spark about one twentieth of an inch in length was obtained. This, of course, discharged the wire,

and the pointer returned to 0, returning, however, almost at once, again to the side of the case. In the hope of getting the deflection within the limits of the scale of the instrument, the quadrants connected with the ground were instead connected with the plus pole of the 100-cell Beetz battery. The deflection under this arrangement would represent the excess value of the air potential over that of the battery terminal. The deflection, however, still remained off the scale, though evidently not very distant. This deflection was maintained all the forenoon. The sky was covered with a low pallium of dark stratus clouds, and the weather was generally muggy and threatening. At three P. M. the kite was sent up as before. The same character of deflection prevailed, and the sparks obtained on connecting the ground with the kite wire were larger and as frequent as in the morning. The shock felt on touching these two wires with the fingers slightly moistened was about the same as one gets from a small-sized Leyden jar. The weather had cleared up, and it was now a clear and pleasant June afternoon. The wind was less steady than in the morning, coming more in puffs. In consequence of this, the kite was less steady, and kept rising and falling. Every time, without exception, when the kite would rise, the needle indicated an increase in the potential; and, on the other hand, as regularly as the kite fell the deflection decreased. When the kite would get apparently within 100 feet of the ground, the deflection would often fall to 8, 10, and sometimes less, of the scale divisions. The movements of the kite were told by watching the movements of the needle, and about as quickly as they could be seen from without.

On June 18, at 2 h. 30 m., P. M., these experiments were repeated. As before, no difference could be detected between the potential of the air a few feet from the ground, and that of the ground. It is to be remembered, however, that in the present adjustment of the electrometer, a difference equivalent to two volts was the minimum difference that could be detected. With the same instrument, adjusted for greater sensitiveness, employing the same methods, I have found in another locality a difference equivalent to a volt and more between a point in air 10 feet from the ground and the ground itself. The sky had been perfectly cloudless for many hours, and was now without clouds, except one or

two very small rounded cumuli in the east. These also in the course of an hour disappeared, and the sky was again cloudless. The height of the kite was determined experimentally, by sending up a carrier on the kite-string, to which was attached thread, with markers at certain distances, made of folded wrapping paper, and of just sufficient weight to keep the thread perpendicular. The distance between the last marker and a level line of sight from the summit was estimated, and added to the known length of thread. My own carrier device having failed, Mr. Willard Gerrish, of the Observatory, suggested a conical-shaped carrier, which answered well. When the kite was about 350 feet high, the needle was deflected off the scale, but did not press against the side of the case of the instrument. When the kite was about 200 feet high, the deflections were in the neighborhood of 10, and variable in character. At this time, then, it was easily possible to notice and record the fluctuations in the value of the potential of the air. At times the needle would start suddenly and swing off the scale; sometimes remaining off, sometimes immediately returning. At other times it would fall to 5, 7, or 8, and either remain at these figures for awhile, or vary greatly therefrom.

At 4 h. 30 m., with a cloudless sky, the kite being at a height of 300 feet, the deflection was 23+. The kite was then pulled down until only about half as high as before, and the average deflection was 15+. At 4 h. 50 m., the kite being about 400 feet high, the length of kite-string being about 700 feet, and the distance between the kite and the ground beneath it about 1,000 feet, there being a deep glen between this and the next hill, the deflection was off the scale, and apparently much greater than when at 300 feet. The sparks obtained by presenting the ground-wire to the kite-wire were larger than before, being about one-eighth of an inch in length. The sky was perfectly cloudless at this time. As the kite remained very steady, it was fastened, and allowed to remain up until near eleven o'clock. The deflection of the needle was observed at frequent intervals, and the needle then brought back to the zero by connecting with the ground.

At eight P. M. the kite was more to the northeast than before, but at about the same elevation. The deflections, however, were much less than during the rest of the day, and for the most part

within the scale limits. The needle kept constantly moving, but with little of the vigor it had previously shown. The character of the deflections may be illustrated by the following record for a single minute:

Time. June 19, 1885, P. M. h. m. s.	Deflection.	Character.	Equivalent in Volts to
8 4 0	24+	Steady.	Over 500
10	10	Variable.	" 200
20	8	Decreasing steadily.	" 150
30	15	Very variable.	" 300
40	15	Variable, increasing and decreasing.	" 300
50	8 to 15	Variable.	" 150
8 5 0	10	.	" 200

The movements of the kite at this time appeared to be very slight, and one would be apt to suppose too small to account for the great potential changes. But it must be noticed that the movements of the kite, as far as they could be made out, were always in the proper direction to correspond with the character of the potential changes; that is, a rise in the position of the kite was attended with an increasing potential, and a fall attended with a decreasing potential.

At 8 h. 20 m. the deflection was very steady in character, and about 18. The potential indicated by that deflection was not sufficient to give a spark. Fifteen minutes later fair-sized sparks could be obtained at very short intervals. At nine o'clock the same condition prevailed. At ten o'clock the sparks were larger and more frequent. The sky during the whole time was cloudless.

The morning of June 19 was cloudless but hazy, and there was not sufficient wind to fly the kite. In the late afternoon, the wind having freshened from the southwest, the kite was raised to an elevation of about 500 feet above the summit. The kite remaining steady, the deflection was beyond the range of the instrument, and evidently greater than had yet been obtained. Large sparks could be obtained every few seconds by presenting the ground-wire to the kite-wire.

The observatory at Blue Hill is provided with a self-registering anemoscope and a self-registering anemometer. It was originally intended to get a record of the potential variations, and compare it with the records of these two instruments. Unfortunately, both had to be returned to the maker for alterations, because of

changes made in the building. It also became apparent that some form of self-recording electrometer was needed. I believe that by means of self-recording instruments the subject of atmospheric electricity can be most advantageously studied. The main purpose in these experiments was to show that it is possible to get some knowledge of the electrical condition of the air at a distance from the ground, with the imperfect means now at our command.

The point of striking interest in these experiments is the obtaining evidence of this high electrical potential in a sky free from clouds. It will perhaps afford an argument against the necessity of considering condensation in explaining the origin of atmospheric electricity.

It may still be questioned whether these experiments prove that the potential of the air is positive, and increases steadily with increase in elevation. The effects observed may possibly be due to the friction of the air against the tin-foiled kite. My own opinion inclines to the former belief. Questions of this nature, however, can be definitely settled by the results of many and long observations. To accomplish these it is indispensable that a self-recording electrometer be devised, so that the records obtained may be directly comparable with the continuous records of the more prominent meteorological conditions.

I am much indebted to Mr. A. Lawrence Rotch, of the Observatory at Blue Hill, for assistance in carrying on these experiments.

ALEXANDER MCADIE.

CAMBRIDGE, MASS.

LITERARY NOTES.

COMPTES RENDUS HEBDOMADAIRES *des séances de l' Académie des Sciences*, Vol. CI, Nos. 1-14, July 6 to October 5, 1885. 4 to pp. 1-680, weekly.

(171) **H. Faye**, *Réponse á la note de M. Mascart sur les grandes mouvements de l'atmosphere*, pp. 19-24, 123-129, and response of M. Mascart, pp. 129-131. M. Mascart, in criticizing M. Faye's meteorological theories, makes certain definite statements in succinct form as to what modern meteorological observations show concerning storms. M. Faye takes issue with him on these grounds, and in the course of his response gives a brief statement of his theories. These we will reproduce elsewhere,

M. Mascart in reply thinks the progressive motion is not so uniform in cyclones as M. Faye requires, and, moreover, while the latter's theory requires the mountains to be especially the field of tornadic activity, Lieut. Finley has shown that those are the regions in the United States which are most exempt.

(172) **Al. Grandval and H. Lajoux.** *Nouveau procédé pour la recherche et le dosage rapide de faibles quantités d'acide nitrique dans l'air, l'eau, le sol. etc.*, pp. 62-64. The authors have simplified the tests for minute quantities of nitric acid until they can be applied by the meteorologist, or student of agriculture. They have found that the air generally contains traces of nitric acid which notably increase during storms. On June 15 with a thunderstorm they found 0.000288 gramme for each cubic meter of air, and on June 16, when a storm threatened, 0.000299 gramme of the acid for each cubic meter. On June 17 in water collected in the first twenty minutes of rain they found 0.00914 gramme per litre; in the next twenty minutes only 0.000948. They also find it in the natural waters of Rheims in proportions varying from 0.005 to 0.201 gramme per litre.

(173) **J. Janssen.** *Spectres telluriques*; pp. 111, 112. M. Janssen announces that the apparatus is prepared for the study of that part of the solar spectrum which is due to the terrestrial atmosphere. The existence of the earth lines in the solar spectrum was announced by him in 1862. He discovered the spectrum of aqueous vapor in 1866, as also the variations of the earth lines with the hygrometric state of the atmosphere. He hopes to be able to proceed with the work now.

(174) **Chevreul.** *Sur le mouvement des poussières abandonnées à elles-mêmes*, pp. 122, 123. Dusts abandoned to themselves in a shallow closed vessel, settle to the bottom, but afterwards undergo various changes in accumulation. These changes are not described here in detail but reference is made to photographs.

(175) **G. Tissandier**, *Sur des expériences de photographie en ballon.* The author was accompanied in an ascent on June 19, 1885, by a photographer who took several negatives at elevations of from 600 to 1100 meters (2000 to 3600 feet.) The views were, in some cases remarkably clear. The experiment shows the possibility of photographing for military topographical, meteorological or other purposes from a balloon. The negatives required an exposure of only one sixtieth of a second. The plates were by Vera prepared with Bacard's gelatino-bromide of iron emulsion.

(176) **Virlet d'Aoust**, *Sur un tremblement de terre partiel de la surface seule du sol*, pp. 189, 190. This earthquake occurred June 24, near Douai, in the Department of the North, on surface underlaid by chalk which in its turn, covered coal. The chalk is 230 meters thick, and the remarkable fact was that the earthquake shock, distinctly felt at the surface, was not felt by the miners in the drifts 278 meters and 334 meters below the

surface. It was evidently a superficial shock, not extending below the chalk.

(177) **H. Faye,** *Suite de la discussion sur les grands mouvements gyratoires de l'atmosphere*, pp. 281–287. The author points out in strong terms the weaknesses of the common theory of cyclones and anticyclones, and sums up his objections in the following queries.

1. If the *trombes* and tornadoes are columns of ascending air, they draw in from below the air which feeds them. Whence, then, comes this air when they touch the soil or the surface of the sea?

2. If the air rises vertically in *trombes* and tornadoes, how is it that the cloud of spray, around the foot, falls back into the sea instead of being drawn up into the spout?

3. If the air is ascending and is drawn from inferior layers in a perfect calm, by what physical or mechanical forces does this ascending column originate its translation, in a determinate direction, with the velocity of an express train?

4. It is certain that tornadoes and water-spouts travel with great speed. If they are columns of ascending air by virtue of what mechanical or physical forces do they travel inclined forwards, never backwards, like the columns of warm air leaving the smoke stacks of our locomotives and steamboats?

5. If the *trombes* or tornadoes are columns of ascending air fed by the motionless air of the lower layers, by virtue of what mechanical or physical causes, do they present at the foot, sharply and without transition, a frightful gyration?

6. If they are columns of air which rise from the ground to the clouds, and which are made visible by the fog they make from a slight lowering of temperature, by virtue of what mechanical or physical laws does their destructive action cease as soon as this fog ceases to reach the ground, as soon as the foot of the *trombe* leaves the earth, to begin again a moment afterwards, when the *trombe* descends anew?

7. If they are ascending columns of air, drawing their force and their supply from the level of the ground, by what physical or mechanical causes, do the tornadoes of the United States (and of France) travel from west to east, or rather from southwest to northeast, and never in the opposite direction? Why is this dreadful gyration always direct, never retrograde?

M. Faye thinks that while on his theory the above questions are easily answered, they are not, and can not be, by the theory of surface origin of tornadoes.

(178) **Mascart.** *Réponse à la communication de M. Faye*, pp. 287–290. M. Mascart strongly supports the prevalent theory. He regrets that M. Faye should confine his attention to tornadoes for the cyclones and anticyclones present greater guarantees of correct observation. He again pro-

pounds the question, why are tornadoes most common on the great plains of the Missouri and Mississippi, and quotes Lieutenant Finley (with a complimentary reference to his competency) to the effect that a suction is generally evident in them.

(179) **G. Sire.** *Nouvel hygromètre à condensation*, p. 312. This is a modification of Regnault's condensing hygrometer.

(180) **P. Tacchini.** *Observation de la corounne solaire, faite sur l'Etna*, pp. 330, 331. The author writes from Rome, under date of July 24, 1885, that Bishop's ring had been re-observed for some time by mountaineers and Alpine climbers, and that early in July he saw it from Etna at an elevation of 3300 meters. It was of a reddish copper color.

He has also noted the reappearance of the twilight phenomena since July 2, 1885. They are less bright than in preceding years. The author suggests that their reappearance is not consistent with the theory that they originated with the Krakatoa eruption.

(181) **Jose J. Landerer.** *Sur l'origine cosmique des lueurs crépusculaires*, pp. 331, 332. The earth had the longitude of the ascending node of Biela's comet on June first. At about that time the twilight colors reappeared. The author therefore attempts to strengthen his theory of the cosmic origin of the colors by pointing this out.

(182) **A Crova** *Sur un enregistreur de l'intensité calorifique de la radiation solaire*, pp. 418, 421. By utilizing thermo-electricity, and a galvanometer, he gets constant records of insolation.

(183) **Houdaille.** *Sur l'évaporation dans l'aire en mouvement*, pp. 429—31. The quantity of evaporation depends on two terms; one is the evaporation in quiet air and this decreases with increase of wind. The second depends on the motion of the air and increases with increase of speed. It is only by taking both terms into account that consistent results can be obtained.

(184) **Ch. Tellier**, (p. 455), describes an apparatus for raising water to a reservoir by means of the heat of the atmosphere. The heat is used to produce ammonia gas from a solution placed between two plates of sheet-iron of large surface. The gas acts exteriorly on a reservoir of caoutchouc containing water. The expansion of the gas forces the water up a tube to a reservoir above it. The gas is then washed up by water and this solution is again employed for producing more gas.

(185) **H. Faye.** *Sur les grains arqués et les typhons*, pp. 460, 466. In this note M. Faye considers the arched squalls (see Vol. I., p. 65, of this *Journal*). Their visible cloud outline may be something that of a rainbow, This arch the author considers only a part of the ring of cloud, and seen by oblique projection. His general argument is that all gyratory storms have a circular form. This is evident in tornadoes and water spouts, and the arched squalls make it evident for the large cyclones.

(186) **Virlet d'Aoust.** *Nouvean tremblement de terre partiel aux environs*

de Douai, p. 487. This was an earthquake on August 5 under the same circumstances as in the previous case No. (176) reported by the same gentleman. The shock, though occurring when work in the collieries was most active, was not felt in the coal-drifts, but was distinctly felt at the surface. It was apparently in the chalk only, not at all in the coal-measures underneath. The thickness of the chalk is 230 meters.

(187) **Hirn.** *Notice sur les rougeurs crépusculaires obserrées à la fin de* 1883, p. 500. M. Hirn followed the colored twilights at his observatory at Colmar and was much surprised to find for them an altitude much above the height of the atmosphere. He thinks that electricity alone would be able to sustain this very fine material, at such a distance from the earth, and then, only in supposing: 1—that the extreme layers of our atmosphere possess quite powerful electricity of their own, and: 2—that this dust has an electricity of the same name.

(188) **S. Arloing.** *Influence du soleil sur la végétabilité des spores du* Bacillus anthracis, pp. 511, 513. The author found that the activity and capacity for propagation of the spores of this dangerous bacillus was suspended or destroyed by exposure to the direct rays of the July sun, according as the insolation was more or less than two hours. If it was less the spores under favorable circumstances would awaken in from eight hours to four days. This destructive property did not belong to any particular part of this solar spectrum but to light taken together. Similar results had been previously obtained from artificial light. The antiseptic properties of light are here shown experimentally and quantitatively. It is also evident that the vitality of these spores is less than had been thought; they can be easily killed, and the attenuation of their virus is a legitimate and promising undertaking.

(189) **H Faye.** *Sur la nature cyclonique des taches du soleil*, pp. 521, 527. This is a response to the objections of S. Tacchini, director of the Observatory of the Roman College to M. Faye's cyclonic theory of sunspots. M. Faye first calls attention to the peculiarities of whirlpools as found by analysis and proven by experiments. He then points out that all the parts of an ærial whirl would not be visible, but that, so far as they would be visible as seen from above, the sunspots would be like them. Apropos of the discussion he mentions the fact that in the books on mechanics and physics there is a regrettable lacuna, so far as relates to whirls.

(190) **S. Arloing,** *Influence du soleil sur la végétation, la végétabilité, et la virulence des cultures du* Bacillus anthracis, pp. 535, 537. The sun's rays are not so destructive to the other parts of this bacillus as they are to the spores, (see second notice preceding). Even in these forms of the plant, however, the sun's rays attenuate their destructive properties.

(191) **Rougerie.** *Sur un appareil producteur du vent*, pp. 568, 569. This is a small terrestrial globe set in rotation, with small weather cocks set on it. It shows distinctly the tradewinds, the line of calms, the chief

ascending and descending currents, and some other features, but does not sufficiently show the temperate variable and the regular polar winds.

(192) **A. d'Abbadie.** *Sur les seismes*, pp. 629, 331. The author describes his apparatus which enables him to take observations something like the nadir observations of the astronomers. By this means he finds, traversing his basin of mercury, many vibrations, so many that it is rare to find it entirely still. The sea near by may cause vibrations when the waves are running high, but they also occur when the sea is smooth.

(193) **George Sire.** *Sur deux hygrometres à condensation*, pp. 638. A farther study of condensation—hygrometers.

(194) **J. Janssen.** *Analyse spectrale de l'atmosphere terrestre*, pp. 649, 651. M. Janssen, director of the observatory at Meudon announces the beginning of the study of the spectral absorption lines due to the earth's atmosphere. In this note he announces some experimental results, obtained by varying the pressure from hydrogen, oxygen, and air. H.

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(195) **Ben Nevis.** p. 17. An extract of an address delivered by Mr. R. T. Omond, superintendent of the Meteorological Observatory at Ben Nevis, before the Royal Society of Edinburg, on some of the results obtained during the two years' residence and work on Ben Nevis.

During the summer and autumn of 1884, some 3,000 or 4,000 tourists climbed the mountain. Storms of exceptional and terrific violence were described. Rainbows were seldom; thunder-storms were rare. The wind was found to have a higher average velocity at night. The air was usually saturated with moisture, but at times was extraordinarily dry.

(196) **Sunlight and the Earth's Atmospheres** pp. 17–20 and 40–44. Lecture delivered at the Royal Institute, April 17, 1885, by S. P. Langley. Prof. Langley stated that if it were not for the atmosphere the sun would appear of a blue color, the reason of its appearing white being due to a large absorption of the blue rays by the atmosphere before they reach the dwellers below. To prove this Prof. Langley made a journey to the top of Mt. Whitney, California, which rises nearly 15,000 feet above the desert below. Here, by means of the bolometer, he proved not only the absorption of the blue rays but that the heat spectrum was very much hotter than had previously been supposed.

(197) **Meterological Instruments.** p. 67. A short account of some instruments exhibited before the Royal Meteorological Society, at its sixth annual exhibition of instruments. This exhibition was devoted to sunshine recorders, and solar and terrestial radiation instruments.

(198) **Sophus Tromholt.** *A Yearly and Daily Period in Telegraphic Perturbations.* pp. 88–89. Dr. Tromholt gives the results of some studies of telegraphic perturbations observed at four stations in Norway and Sweden.

He finds that the perturbations have two maxima and minima of intensity during the year, the maxima coinciding with the solstices and the minima with the equinoxes, hence presenting an annual period identical with that of the aurora. He also finds a well marked daily period with greatest maximum 8 to 9 P. M., with a smaller maximum at 10 to 11 A. M. and a minimum 1 to 2 P. M.

Sophus Tromholt. *A Note Relating to the History of the Aurora Borealis.* p. 90. Dr. Tromholt gives a few extracts from early writers (1500 to 1600 A. D.) regarding the aurora.

(200) **The Influence of Forests on Climate.** p. 115. A review of an article by Herr A. Wœikof, contained in the third number of *Petermann's Mittheilungen*, 1885. The general results laid down by the writer are that, "As a general rule it may be laid down that in warm seasons, as between forests and places close at hand which are treeless, (1) the temperature of the earth and air are lower in the former, (2) their variations are less, (3) the relative humidity is greater.

Wœikof found from observations in the vicinity of Nancy that in winter as well as in summer the forest got more rain than the surrounding regions.

(201) **J. Y. Buchanan.** *Observations of the Temperature of the Sea and Air, made during a voyage from England to the River Plata in the steamship "Leibnitz."* pp. 126 to 130.

This writer found in the sea surface water well marked diurnal maxima and minima of temperature.

With two exceptions, the temperature of the sea was always found higher than that of the daily mean temperature of the air, and only very seldom was it exceeded by that of the air at the hottest time of the day.

(202) **The Meteorology of Bombay.** pp. 170-171.

A review of the annual report of 1883, made from the Government Observatory, Bombay.

(203) **J. W. Clarke.** *On a Radiant Energy Recorder.* p. 233. The principle of the instrument described depends upon the evaporation of water *in vacuo*, and its indications are therefore readily expressible in heat units, It seems to be of a similar pattern to that described in the *Journal*. vol. 1, p. 33.

(204) **The Aurora.** pp. 274 to 276 and 348 to 352.

The article is a review of Mr. S. Tromholt's book, "Under the Rays of the Aurora Borealis"; edited by Carl Siewers. Mr. Tromholt deals mainly with the results obtained by an organized effort to study the aurora during the existence of the International Polar Research Expedition of 1882-83. The most important conclusion arrived at was that there is an auroral zone which exists as a ring around the magnetic pole, or some point near it, and that the auroral zone moves northwards and southwards daily, yearly, and eleven yearly. Towards the autumnal equi-

nox the zone increasing in diameter moves southward, so that more southerly stations have a maximum of auroras, while the more northerly have a minimum; the zone then moves northward, reaching its most northern limit about the time of the solstice. After this it again moves southward, being in its most southern position at the vernal equinox, when the movement is again in a northerly direction.

The auroral zone has a somewhat similar movement during the sunspot period. At sunspot maximum the auroral zone reaches its farthest points south, giving a maximum of auroras to more southerly stations. At sunspot minimum it reaches it farthest point north, giving a minimum of auroras to more southerly points.

(205) **Forecasting by means of Weather Charts.** pp. 392-4. A review of Hon. Ralph Abercromby's book on this subject. See this JOURNAL, p. 543, Vol. I.

(206) **The International Meteorological Committee,** pp. 501-2. The committee held its third annual meeting in Paris. The principal reports and discussions were on cirrus clouds; Atlantic telegrams to Europe of American weather; the reduction of barometric pressure to sea level and to 45° lat.; and discussions in regard to the times of international observations and reductions.

(207) **Charles Meldrum, F. R. S.** *On a Supposed Periodicity of the Cyclones of the Indian Ocean South of the Equator.* pp. 613-4. In a paper written thirteen years ago, the writer endeavored to show that there were grounds for supposing that the cyclones of the south Indian Ocean went through oscillations in their number and intensity, apparently corresponding with the eleven year period of solar activity. He now states that the extended and carefully gathered statistics of the last ten years show a fluctuation in the number of cyclones closely resembling the fluctuation in the number of sun spots.

(208) **The Heights of Clouds.** pp. 630 to 631.

"From the Upsala Observatory comes an account of fairly exact measurements of the heights of clouds during the summer of last year." "On a couple of pillars, about 450 yards apart, and placed on an approximately N. and S. line, a couple of theodolites were erected, the stations being connected by telephones." "The two observers, each at a theodolite, agreed as well as they could on the point in the cloud to be observed, and, at a particular time, fixed on in advance, brought the cross wires on this somewhat indefinite spot, and then read their instruments, noted the time of observation, described the cloud, and if possible sketched it."

The following table gives the height of the different characters of cloud:

Stratus	625 meters.	Lower alto-cum	1,988 meters.
Nimbus (lower)	1,115 "	Higher " "	4,242 "
" higher	2,185 "	Cirro-cum	5,513 "
Cumulus, top	1,690 "	Cirrus	6,823 "
Cumulus, base	1,307 "		

H. H. C.